Note on the Recent Determination of the Longitude of Paris.
By H. H. Turner, M.A., B.Sc.

In the year 1887 a request was made to the Astronomer Royal by the officers of the French Geodetic Survey that the longitude Paris-Greenwich should be redetermined with all the accuracy attainable by modern methods. This longitude was determined telegraphically under the direction of M. Le Verrier and Sir George Airy in 1854, both stars and signals being observed by the eye and ear method. The observers were M. Faye, of the Paris Observatory, and Mr. Dunkin, of the Greenwich Observatory, who were interchanged once. The instruments used were the large transit circles of the two observatories. The result found for the difference of longitude between the Greenwich transit-circle and the meridian of Cassini (Paris) was $9^{m}\ 20^{s}{\cdot}51$; and the accordance of the twelve nights of observation was remarkably good: it was quoted by Sir George Airy, in a letter to Sir John Herschel, as sufficient to justify the adoption of the final result, although this differed by about 1^{s} from the previously-assumed longitude, $9^{m}\ 21^{s}{\cdot}57$, determined by rocket signals in July, 1825. (*Phil. Trans.* 1826, Part II.)

But there has since been reason to doubt the correctness of this result. In 1872 the same longitude was found by J. E. Hilgard, of the U.S. Coast Survey, to be $9^{m}\ 20^{s}{\cdot}97$ (U.S. Coast Survey: Report for 1872); and since then various indirect determinations (such as the combination of the longitude Vienna-Greenwich with Paris-Vienna) have indicated that the result of 1854 is too small. The following is a list of all telegraphic determinations published up to 1888:—

Longitude Paris (Cassini's meridian)—Greenwich (Transit Circle).

I. Direct.

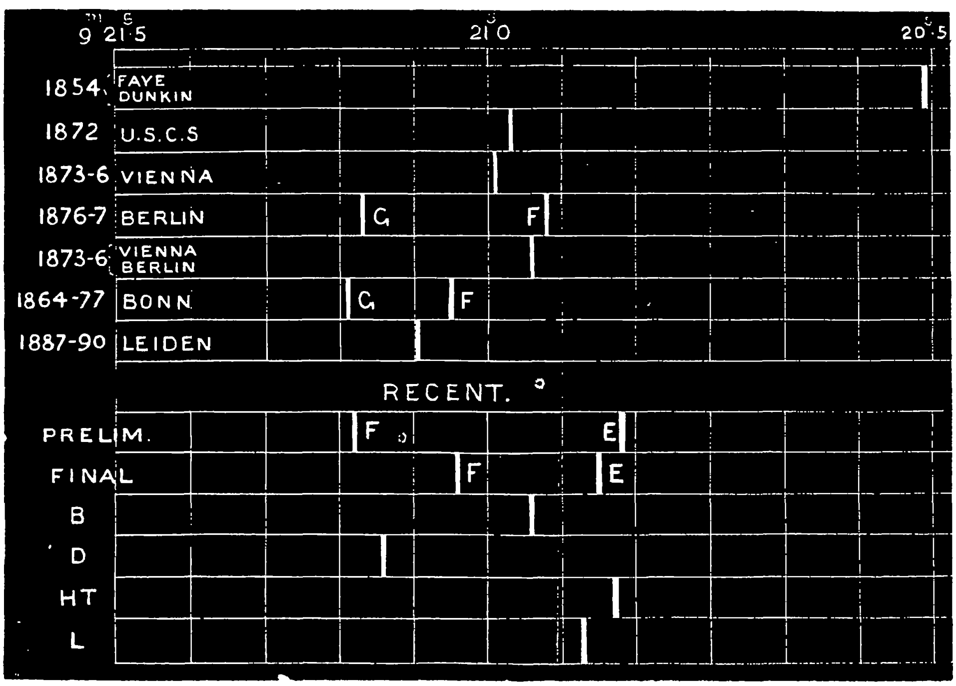
	Result. m s	Date.	Reference.
Paris-Greenwich	9 20·51	1854	*Monthly Notices, R.A.S.* vol. xv. p. 124 (reduced to Cassini's meridian).
Paris-Greenwich	9 20·97	1872	U.S. Coast Survey, 1874, p. 180 (reduced to Cassini's meridian).

II. Indirect.

	Result. m s	Date.	Reference.
Vienna-Greenwich	65 21·21	1876	Assoc. Géodésique. Report on Longitude, by Bruhns (1880).
Paris-Vienna	56 0·22	1873.	
Paris-Greenwich	9 20 99		
Berlin-Greenwich	53 34·91	1876	Assoc. Géodés. (1883). Report by Bakhuyzen.
Paris-Berlin (F.)	44 13·99	1877	Comptes Rend. 1878, Feb. 4.
Paris-Greenwich (F.)	9 20·92		

	Result. m s	Date.	Reference.
Berlin-Greenwich	53 34·91	1876	*ut supra.*
Paris-Berlin (G.)	44 13·75	1877	Pub. Preuss. Geodät. Institut. 1877, p. 147.
Paris-Greenwich (G.)	9 21·16		
Vienna-Berlin	11 46·26	1876	Assoc. Géodés. (1883).
Berlin-Greenwich	53 34·91	1876	Assoc. Géodés. (1883).
Paris-Vienna	−56 0·22	1873	*ut supra.*
Paris-Greenwich	9 20·95		
Bonn-Greenwich	28 23·31	1864	Assoc. Géodés. (1880).
Paris-Bonn (F.)	19 2·26	1877	Assoc. Géodés. (1880).
Paris-Greenwich (F.)	9 21·05		
Bonn-Greenwich	28 23·31	1864	
Paris-Bonn (G.)	19 2·12	1877	Pub. Pr. Geod. Inst. (1877)
Paris-Greenwich (G.)	9 21·19		
Greenwich-Leiden	17 56 30	1880	Assoc. Géodés. (1883).
Paris-Leiden	8 35·21		Provisional.
Paris-Greenwich	9 21·09		

The determinations Paris-Berlin and Paris-Bonn in 1877 were made independently by French and German observers, whose results are distinguished in the above table by the letters (F.) and (G.) A comprehensive view of these results is afforded by the upper portion of the subjoined diagram.



It is thus apparent that a direct redetermination of this longitude was a matter of great interest and importance. The Astronomer Royal accordingly entered into communication with the late General Perrier, then Director of the "Service Géographique de l'Armée Française," and with Admiral Mouchez, the Director of the Paris Observatory, as to the programme of observations and the date on which the operations could be commenced. It was decided that the summer of 1888 should be devoted to the work; and at this point negotiations were interrupted by the sudden and lamented death of General Perrier, on February 20, 1888. They were shortly afterwards renewed by Col. Derrècagaix, General Perrier's successor. It was arranged that M. Loewy (representing the Paris Observatory and the Bureau des Longitudes) and Commandants Bassot and Defforges (representing the Service Géographique) should pay a visit to Greenwich and settle the definitive programme in consultation with the Astronomer Royal. This visit was paid on June 15, 1888, and following days. M. Loewy reported that the Paris Observatory would not be able to take part in the work, which would thus be shared by the French Service Géographique and the Royal Observatory, Greenwich. It was decided that two observers should be appointed by each of these; and that the French observers should be Commt. Bassot (B) and Commt. Defforges (D); and the English Mr. Turner (H T) and Mr. Lewis (L). B and D were to use two French portable transit instruments by Brunner Bros., such as they had already used for other longitudes; and H T and L two of the portable transits by Troughton and Simms in the possession of the Greenwich Observatory. The observers were to be interchanged twice as follows:—

1st Series: 3 full nights, or the equivalent in half-nights.
D and L at Greenwich, B and H T at Paris.
2nd Series: 3 full nights, or equivalent.
B and H T at Greenwich, D and L at Paris.
3rd Series: 3 full nights, or equivalent.
B and H T at Greenwich, D and L at Paris.
4th Series: 3 full nights, or equivalent.
D and L at Greenwich, B and H T at Paris.

All four observers were to exchange signals automatically on their respective chronographs each night [in the order B and D, B and L, H T and L, H T and D for the 1st and 4th series, and the order D and B, D and H T, L and H T, L and B for the 2nd and 3rd series], so as to give 4 determinations of personal equation, two and two, and to eliminate small differences in the two chronographs at each station. Later in each complete evening the exchanges B—D and H T—L only were to be repeated, so that the mean epoch of these pairs of exchanges should be nearly the same as that of the time-determinations. The Paris station was at the Observatoire du Dépôt de la Guerre, Montsouris, where one French and one English instru-

ment were mounted on two piers 12 feet apart, the English instrument being to the east; and the Greenwich station the front court of the Royal Observatory, where two transit-huts were erected and two piers built, that for the French instrument being 41 feet, and that for the English instrument being 19 feet, west of the transit-circle. The four chronographs were kindly lent by the French Service Géographique, and had been in use for other longitude operations. A spare wire was courteously placed at the disposal of the Astronomer Royal each night from eight to twelve by the Submarine Telegraph Company; and the French and English Post Offices provided the services of clerks at Montsouris and Greenwich during these hours, and the use of wires from Paris to Calais and from Greenwich to London, so as to give direct communication between Montsouris and Greenwich. Operations were commenced on September 21, 1888, when Commt. Defforges arrived at Greenwich to co-operate in preparing the galvanic connections and to mount his instrument. Mr. Turner left for Paris on September 23. The following is an account of the course of the work, P denoting Paris and G Greenwich:—

September 25.—Cloudy P and G: exchange of practice signals.

September 26.—Cloudy P and G.

September 27.—Partly clear: instruments adjusted to meridian.

September 28.—Cloudy P and G.

September 29.—Cloudy P and G.

September 30.—Partly clear: one exchange: half-night.

October 1.—Partly clear: one exchange: half-night.

October 2.—Clear G: cloudy P.

October 3.—Partly clear: one exchange: half-night.

October 4.—Cloudy P and G.

October 5.—Quite clear P and G: two complete exchanges: full night.

(End of 1st Series.)

October 6.—HT returned to Greenwich. Clear G: cloudy P.

October 7.—D and L went to Paris. Clear G and P.

October 8.—B arrived at Greenwich. Clear G: raining P.

October 9.—Clear G: raining P.

October 10.—Cloudy P and G.

October 11.—Quite clear P and G: two exchanges: full night.

October 12.—Cloudy P and G.

October 13.—Clear P and G: two exchanges: full night.

October 14.—Clear P and G: two exchanges: full night.

October 15.—Clear P and G: trouble with batteries G: one exchange: half night.

(End of 2nd Series.)

October 16.—Cloudy P and G.
October 17.—Clear P and G: two exchanges: full night.
October 18.—Cloudy G: clear P.
October 19.—Clear P and G: two exchanges: full night.
October 20.—Clear G: only partly P: half night.
October 21.—Clear P and G: two exchanges: full night.

(End of 3rd Series.)

October 22.—B and H T went to Paris.
October 23.—D and L went to Greenwich: clear P: only partly G: half night.
October 24.—Cloudy P and G.
October 25.—Cloudy P and G.
October 26.—Cloudy P and G.
October 27.—Clear P and G: two exchanges: full night.
October 28.—Raining G: partly clear P.
October 29.—Raining G: partly clear P.
October 30.—Partly clear: one exchange: half night.
November 1.—Cloudy P and G.
November 2.—Cloudy P and G.
November 3.—Cloudy P and G.
November 4.—Cloudy P and G.
November 5.—Cloudy P and G.
November 6.—Cloudy P: clear G.
November 7.—Cloudy P and G.
November 8.—Cloudy P and G.
November 9.—Partly clear: one exchange: half night.
November 10.—Cloudy P and G.
November 11.—Cloudy P and G.
November 12.—Cloudy G: clear P.
November 13.—Clear P and G: two exchanges: full night.

(End of 4th Series.)

It was decided that the English observations and the French should be worked up and published separately; and the following account is chiefly confined to the English observations.

Instruments used, etc.

Transits B (at Paris) and C (at Greenwich) of the series used in Transit of *Venus* 1874. Clear aperture $3\frac{1}{2}$ inches, focal length $36\frac{1}{2}$ inches. The pivots were re-turned before the operations and found sensibly circular, and equal.

Clocks.

At Paris, Breguet No. 2, placed in the observing-room and subjected to considerable fluctuations of temperature.

At Greenwich, the Sidereal Standard, through the medium of two relays.

Instrumental Constants, etc.

Considerable labour was requisite to obtain a satisfactory determination of the values of the striding-level scales, which were found to be by no means uniform. A careful calibration, however, reduced the limits of possible error to very small amounts. The value of $1^{\text{div.}}$ ranged from $0''\cdot73$ to $1''\cdot35$ for the level of Transit B, and from $0''\cdot92$ to $1''\cdot57$ for that of Transit C.

Chronographs, etc.

The chronographs and the other automatic apparatus were of the pattern fully described in the "Mémorial du Dépôt Général de la Guerre," Tome XI., Section A, p. 20.

Instrumental Errors.

The collimation-error was determined both by reflexion observations from mercury and from observations of the same polar star in reversed positions of the instrument. No sensible systematic difference was found between the two methods for either instrument. [The actual mean systematic differences found for Transits B and C were $+0''\cdot03$ and $-0''\cdot15$ respectively.] Some considerable trouble was caused at the beginning of the operations by the object-glasses almost simultaneously becoming loose in their cells. Trial observations with the instruments had been quite satisfactory. Fortunately, the discovery was made, and the defect remedied, before any actual longitude work had been done. Messrs. Troughton & Simms, to whom the instruments had previously been sent for examination, are unable to account for the occurrence.

The level error was determined both by reflexion observations and by the striding level. The systematic differences between the two methods were found to be $-0''\cdot13$ and $-0''\cdot28$ for B and C respectively.

The azimuth-error was determined by observation of one or more of 12 polar stars culminating between $19\frac{1}{2}^{\text{h}}$ and $3\frac{1}{4}^{\text{h}}$ sidereal time. It may be remarked that only one of the more frequently observed stars was observed at lower culmination: so that only 46 transits out of 262 were sub-polar. But though this may have some bearing on the absolute clock-errors at the two stations, it does not affect the longitude, the same stars (nearly) being observed at both stations. The places of the polar stars were adopted from recent Greenwich transit-circle observations.

Stars for Clock-error.

The stars were selected from a previously arranged catalogue, and the places adopted were corrected by means of the observations themselves, according to the method explained in the paper in "Mém. Gén. du Dép. de la Guerre" cited above. The mean of the corrections to ephemeris indicated by the results of all four observers was adopted for use.

Clock-rate.

The operations were arranged so that the mean epoch of the signals was as nearly as possible the same as the mean epoch of the star-transits on each night, and thus if the clock-rate be uniform during the operations on each evening, its magnitude has no sensible influence on the result for longitude. Moreover, the rate of the Greenwich clock was very small throughout, never reaching $0^{s}\cdot2$ per day. But it was found that the rate of the clock used at Paris, which was subjected to considerable variations of temperature, was sensibly different at different times of the day. For the present investigation, the rate was assumed uniform for the period of observation on each night, and was determined independently by the English and French observers from comparison of the later star-transits with the earlier. The mean of their results was adopted for use; but the experience gained suggests the advisability of using a more carefully protected clock for longitude operations; for it is not at all clear that the assumption of a uniform rate from sunset to midnight (the usual period of observation for longitude work) for a clock known to have a diurnal inequality, is justifiable; and the above method of eliminating clock-rate by making coincident the mean epochs of signals and time-determination is not only sometimes difficult in practice, but is based entirely on this assumption of uniformity of rate.

Signals.

The exchanges of signals were made automatically, and the pairs of chronographs at each end were combined in the three possible different ways. The differences between different sets ranged over nearly $0^{s}\cdot10$, this quantity being made up of errors resulting from changes in the state of the cable, in clock-rate, and others more or less accidental. As regards changes in the cable it may be remarked that the apparent time of transmission of the current deduced from comparison of signals sent in opposite directions, ranged from zero to $0^{s}\cdot085$ for single sets of signals (40 each way in each set); or, if we compare the mean of different evenings, from $0^{s}\cdot021$ to $0^{s}\cdot067$.

Results for Longitude.

A preliminary discussion of the observations of the English and French observers, who made their computations independently, showed a considerable difference—about $0^{s}\cdot4$—between their results for longitude. Such a large quantity as this was at first attributed to some mistake in computation, and all the figures were subjected to a careful scrutiny. But although one or two small matters were noticed which would diminish this difference (the discussion of which, however, belonged more properly to the final reduction), no serious mistake was discovered. It was then arranged that Commt. Defforges should visit Greenwich, bringing with him the French reductions, to discuss the discre-

pancy in concert with the English observers. This visit was paid in June, 1890. Another thorough examination of the reductions was made without result, but the discrepancy was localised in the time-determinations at Greenwich, where the English and French results were found to differ by a much larger quantity than the known difference of personal equations of the observers would explain. On further inquiry it was found that the results for level-error of the French instrument as obtained with the striding level differed systematically by 3″ from those obtained by reflexion observations from mercury. Only a few of the latter had unfortunately been made, but there could be no doubt about the discrepancy, which was confirmed in every case throughout the observations. The only difficulty was to explain this discrepancy on any assumption of instrumental defect. No satisfactory assumption has yet been thought of; but the fact remains that if the nadir observations were considered correct, and the striding-level observations consequently erroneous by the amount indicated, the French time-determinations would be brought into excellent accord with the English. In the absence of any direct confirmation of this view, however, the French observers did not feel justified in making this assumption; they decided rather to take the mean between the discrepant determinations of level-error; and consequently their results still differ from those of the English observers by half the amount in question—viz. about $0^s.16$.

In the final discussion of the results there arises the question what weight shall be given to different nights. It is usual in such cases to make an elaborate determination of probable error for the various elements, and form weights accordingly; but in the present case a procedure was adopted which throws some light on the practical utility of such investigations—viz. various extreme suppositions were made in different directions, such as (1) that each evening should have equal weight whatever the number of observations; (2) that the weight should be directly proportional to the number of exchanges of signals, or (3) to the number of star-transits observed, &c. The first of these suppositions undoubtedly gives too much weight to the more unsatisfactory nights when little was done, and the second and third too much to the more complete nights. The truth should lie between them. But it was found that the results for longitude were practically the same on any of these suppositions, the whole range in such results being barely $0^s.01$, and the mean of all is thus practically definitive. The following is the series of results obtained on eight of these different suppositions from the English observations:—

m s	m s
9 20.598	9 20.611
.593	.599
.594	.603
.595	.605

It was thus not considered necessary to proceed further with the discussion of weights.

The final result for difference of longitude between the two instruments used by the English observers is $9^m\ 20^s{\cdot}60$, which, reduced to the meridian of Cassini and the Greenwich transit-circle, is

$$9^m\ 20^s{\cdot}85.^*$$

The French reductions are practically complete, and their result will be $9^m\ 21^s{\cdot}04$ within a very small quantity. If the full correction to level indicated by nadir observations were adopted, this would be about $9^m\ 20^s{\cdot}88$, agreeing well with the English determination.

It may be remarked that the continuity of observations for time at Greenwich with the transit-circle, and with the same clock as was used by the longitude observers, affords a means of deducing the longitude from the observations of each observer independently. Thus the observations he made at Greenwich, when compared with those of the transit-circle, give his personal equation referred to the standard observer. On crossing to Paris, his time-determination can thus be directly compared, by means of the signals, with that made at Greenwich with the transit-circle. The results obtained in this manner would be as follows:—

			m	s
B	...	...	9	20·94
D	...	...		21·14
H T	...	...		20·83
L	...	...		20·87

It is to be remarked that the systematic correction to level-error from nadir observations was applied to the observations of B and D alike, although B did not make any nadir determinations. This may account for the discrepancy between their results.

* Transit C was 19 feet, or $0^s{\cdot}02$ W. of the Greenwich Transit-Circle; and Transit B was 12 feet, or $0^s{\cdot}01$ E. of the Montsouris meridian, which is $0^s{\cdot}29$ W. of the meridian of Cassini.

A List of Published Lunar Sketches and Photographs arranged according to the Sun's Position. By A. Marth.

In a paper published in vol. xlviii. of the *Monthly Notices*, containing my ephemeris for physical observations of the Moon for the nine lunations from the middle of April to the end of 1888, I mentioned that I intended to examine all the lunar sketches in the library of the Royal Astronomical Society, and to prepare a properly-arranged list of those which deserve not to be left out, and which are at least approximately timed. The times required for the purpose are obviously the times when the outlines of the shadow are drawn. If these are properly recorded, it is easy, with the help of similar ephemerides and of some data in the list of sketches, to find (to about a minute) the corresponding times in other lunations, when the sun reaches the same zenith-distances at the sketched objects, and thus to connect the phenomena, in whatever lunation they may have been observed. If, instead of these times of drawing the shadows, only vague indications are given that the sketches have been made between such and such hours, the evidence supplied by the drawings is affected by a corresponding vagueness and weakened accordingly. An obvious difficulty arises, if the required times are not attached to the sketches themselves, but have to be searched for through pages of text, when they may perhaps be found mentioned merely by the way as apparently unimportant, or also may be searched for quite in vain. Some series of sketches have also to be disregarded, till it shall please the sketchers to state what times are those which they give.

The strangest fact, however, is, perhaps, that in the library of the Royal Astronomical Society there is a considerable number of lunar photographs, and that a number have also been published, without the slightest indication of the times when they were taken. The greater part of the valuable evidence which these photographs might furnish for the advance of selenography is thus practically lost. How is such unworkmanlike thoughtlessness to be accounted for?

Under the circumstances I give now only a portion of the intended general list of published sketches and photographs.

The first column, headed "Sun's Colongitude," and containing two terms, supplies, for any value of the Sun's selenographical latitude, the colongitude which the Sun must reach in order to be at the same altitude above the horizon of the sketched spot as at the assigned time of the sketch. By reference to the ephemeris the corresponding time may thus be found to about a minute.

Then follow the Sun's colongitude and latitude at the time of the sketch or photograph, the name of the spot, the assigned